U.S. DEPARTMENT OF COMMERCE
National Oceanic and Atmospheric Administration
National Ocean Service
Office of Response and Restoration
Coastal Protection and Restoration Division
c/o EPA Region X (ECL-117)
1200 Sixth Avenue
Seattle, Washington 98101

January 31, 2006

Eric Blischke
U.S. Environmental Protection Agency
Oregon Operations Office
811 SW Sixth Avenue
Portland, OR 97204

Chip Humphrey
U.S. Environmental Protection Agency
Oregon Operations Office
811 SW Sixth Avenue
Portland, OR 97204

Dear Chip and Eric:

This letter provides **NOAA's comments on the Draft Ecological Preliminary Risk Evaluation for the Portland Harbor Superfund Site**. The document, prepared by Windward Environmental LLC for the Lower Willamette Group, is dated September 9, 2005. NOAA's comments are presented as follows: general comments; specific comments; comments on tables; suggestions and comments on figures; and suggested editorial changes.

General Comments:

NOAA's review was focused primarily on the fish tissue TRV sections in Volume 1 of 4; Appendix A and B, Volume 3 of 4; and the oversized tables, Volume 2 of 4. We also quickly reviewed the Biota Sediment Accumulation Factors (BSAF) Table 5-1. With respect to the latter, the PRE document does not always explain how the numbers were derived. Given that it is important to be able to replicate these numbers, the process for deriving BSAFs must be transparent.

With respect to the TRVs, NOAA referred to an email from Burt Shephard of EPA dated May 24, 2005 in which EPA recommended the utilization of a tiered approach using combined aquatic data to develop tissue TRVs. It appears that the LWG has used the tiered approach, but we cannot determine fish and invertebrate LOECs were combined as recommended by EPA. (Combined datasets used for the 5th percentile calculations were either not provided or we were unable to find them. Therefore, we were unable to evaluate completely the LWG's approach.

Specific Comments:

Volume 1 of 4: page 16 Section 4.1.1 – Aquatic Tissue Residue TRVs Tier 1. Using a 5th percentile is not a species sensitivity distribution (SSD) and should not be called one. A SSD involves a more complicated calculation using a cumulative distribution function and then calculating a hazardous concentration for 5 percent of the species. The text should be changed to reflect the method used to calculate a 5th percentile, and the reference to performing a species sensitivity distribution needs to be removed.

Table 4-1 Tissue residue TRVs for aquatic organisms – We could not find the specific data used or how the calculations were made for 4 contaminants whose source is Appendix B; 5th percentile literature LOEC (TCDD, total PCBs, total DDTs, and arsenic). In the text of Appendix B in section 3.11 it states that the selected TRV for TCDD is the lowest LOEC or 1.95 pg/g. We assume that Appendix B is presenting the former TRV approach and was not altered to reflect EPA's recommendation. If the LWG does not want to alter the text in this volume, text should be inserted in section 4.1.1 of Volume 1 to explain that the original selected fish TRVs developed in Appendix B and Table 4 of Appendix B are not being used in this PRE, but are being retained for possible future use. However, since Appendix B separates the fish, decapod and bivalve TRVs, it is not clear if these data were combined when a 5th percentile was taken. In addition, Appendix B is missing Figures 3-8. Some of the contaminant figures for fish have a 5th percentile line drawn on them, but the exact value is not identified, so we cannot tell if this value is what is reported in Table 4-1.

If the purpose of EPA's approach was to make sure the tissue TRVs are conservative, then it would be helpful if Table 4-1 presented the TRVs calculated by the different methods (i.e. lowest LOER from Table 1-1; 5th percentile all aquatic species; Dyer et al. 2000, and the BCF calculation). For example, while most of the TRVs are lower than the previous version, some important contaminant TRVs are now higher. TCDD went from 1.95 pg/g to 90 pg/g; PCBs went from 520 µg/kg to 720 µg/kg; and mercury went from 0.23 mg/kg to 0.46 mg/kg.

For mercury, the value from the Dyer paper is used because, it is stated, there were not 20 studies available, only 18. However, the Dyer et al. paper is from 2000 and does not include more recent high quality mercury studies. Therefore, 0.25 ppm reported in footnote b of Table 4-1, which was the calculated 5th percentile from LWG's literature review, is a better number because it includes more recent studies than the 0.46 ppm in the Dyer et al. paper. NOAA recommends using 0.25 ppm for the mercury TRV. In addition, we think it would be useful to see the HQs recalculated for mercury using the lower number.

Becuase PCBs were determined to be a risk driver in the PRE, we believe it is not worth recalculating the PCB HQs. NOAA recommends that the PCB TRV be revisited in the BERA.

The lower TCDD TRV of 1.95 pg/g is very uncertain. We do not recommend that it be redone or that it be carried forward into the BERA.

Table 5-1, Calculation of BSAFs – How were the BSAFs calculated from the USACE database? LWG says they use a 90th percentile BSAF if there are more than 4 data points. The ORNL 1998 reports a 90th percentile BSAF, but the USACE reports a grand mean. For example, the grand mean for the PAH BSAFs for all aquatic organisms is 0.495 in the USACE database, LWG reports 0.14 in Table 5-1. The USACE dataset is a mixture of individual and average BSAFs and mixes fish and invertebrates. What decision logic was emplyed and what data issues were subsequently resolved to develop the BSAF numbers from this dataset? Was the same approach taken for each chemical?

If a BSAF was not available, a default value of 1 was used for the organics. What is the justification for choosing 1 for both the metals and the organics?

Section 6.1.11, Hazard Quotients for Antimony – Section 6.1.11 provides a list of chemicals that were excluded from the initial list of COPCs because "the HQs associated with these chemicals were low, and the uncertainty high . . ." However, the antimony tissue residue HQ for the white sturgeon was 13.3. When you consider that an HQ of 1 is the standard break point indicating a potential hazard, an HQ of greater than an order of magnitude of this break point is not, on the face of it, insignificant. While the exclusion of antimony from the COPC list may be warranted, it deserves a little more justification/explanation as to why it should be excluded. Examples: sediment and water data don't show elevations significantly above background; only a single tissue sample indicated a problem. If the elevated hazard quotient was the result of modeling then it could be explained by uncertainty. But since it results from an actual tissue measurement it needs some explanation.

NOAA appreciates the opportunity to provide these comments. Please let me know if you have any questions.

Sincerely,

Robert Neely
NOAA Coastal Resource Coordinator

cc: Alyce Fritz, NOAA / NOS / CPRD (by email)
Nancy Munn, NOAA / NMFS / HCD (by email)
Nick Iadanza, NOAA / NOS / DAC (by email)
Katherine Pease, NOAA/GCNR (by email)
Chip Humphrey, USEPA (by email)
Eric Blischke, USEPA (by email)
Rene Fuentes, USEPA (by email)
Jean Lee, Environmental International (by email)